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Discussions and Replies — Session VII

Multiple Authors

DISCUSSIONS AND REPLIES SESSION VII

Discussion on paper titled: "Effects of build environment on "free-field" motion for very soft, urbanized sites", by Pierre-Yves Bard et. al., Paper No. 7.04

By: J. M. Ferritto Naval Facility Engineering Center, Port Hueneme, California.

The author investigates the cause of the long duration shaking in the Mexico City basin from the 1985 Guerrero-Michoacan event. They note that the presence of buildings in an urbanized environment can influence response especially at soft sites. The irregularities at the free surface of a soft layer and bedrock can change the dispersive characteristics of Love and Rayleigh waves. The authors use a 2-dimensional model simulating typical buildings on a horizontally layered half-space to conduct a parametric study, varying building separation and soil depth. They conclude that ground motion at distances away from the structure which would normally be thought of as "free-field" can be influenced by the structure changing the fundamental period and lengthening the duration of the motion. They conclude that the basic effects of buildings in Mexico City was to diffract waves back into the soil which propagate as guided waves in the clay layer. The building mass was "not a crucial factor in this phenomenon."

It would seem that based on this paper, we should examine some of our recording stations both on soft and on stiff geologies. It is expected that the presence of a high plasticity clay layer responding in a more elastic manner with less damping than other soils would tend to significant factor this effect. It would be of interest to see if buildings have as significant an influence in stiffer geologies and at what distance does a "free-field" condition actually exist.

Discussion on paper titled: "Effect of built environment on "free-field" motions for very soft, urbanized sites", by P.-Y. Bard & A. Wirgin (Paper 7.04)

By: Francisco J. Chávez-García, Instituto de Ingeniería, UNAM, Mexico.

The authors present a numerical study of the effect of soil-structure interaction on ground motion near tall buildings. Their model is a periodic succession of rectangular, homogeneous blocks representing the buildings in a city. These blocks rest in welded contact on a layer over half-space model of subsoil. The authors explore the effect of the different parameters that have a strong effect in the problem such as layer thickness and frequency, building size, and space between buildings.

The research reported here is very thought-provoking and its purpose is a better understanding of ground motion on extremely soft soils. The soft layer overlying the half-space represents the very soft clay of lacustrine origin that has been held responsible for large damage observed during the September 1985 earthquakes in Mexico (in all of their examples, S-wave propagation velocity of the soft soil is 60 m/s). They make explicit reference to the case of Mexico City, where subsoil conditions may really approach the extreme cases computed by Bard & Wirgin. In this context, it must be underlined that the authors did consider a realistic value of the anelastic attenuation factor Q for the very soft soil layer. Any realistic simulation of ground motion for Mexico City must indeed consider this parameter.

Some of the limitations of this study are signaled by Bard & Wirgin themselves. It is indeed difficult to reduce the 3D geometry of building distribution in any city (even North-American cities) to a periodic assembly of regular blocks in a 2D geometry. Additionally, as mentioned also by the authors, it would be more interesting to observe whether such effects could be present for in-plane motion, where a priori we could expect larger soil-structure interaction.

There is another serious limitation to the study by Bard & Wirgin that was not risen by the authors. This comes from the fact that foundations were neglected in their study. High-rise buildings built over such soft soils as included by the authors in their models must include a foundation. In Mexico City, current practice favors foundations on friction piles for high-rise buildings. It seems reasonable to expect significant changes in the results presented by Bard & Wirgin, were the foundations of buildings taken into account. However, it seems difficult to predict a priori in what sense would the results change. On the one hand, the buildings would be less excited by seismic motion in the clay layer. On the other hand, once the building is in vibration, there could be a more efficient mechanism to transmit motion to the soft soil layer.

A final comment concerns the very large soil-structure interaction effects predicted by the model of Bard & Wirgin. Their results suggest very significant modifications both on the motion of the building and on the motion of the "free-field". For example, Figure 2 indicates that soil-structure interaction may affect the natural period of the structure (= 0.5 Hz, model a1) by a factor of more than 2.3. However, empirical measurements of natural period of structures in Mexico City suggest that soil-structure interaction may affect dominant period of buildings by a factor between 30 and 40% (Muria-Vila & González-Alcorta, 1994). Therefore, the effects of soil-structure interaction on ground motion may be largely overestimated due to the simple modeling used by the authors.

We hope to see soon the continuation of this study, with a gradual abandonment of the limitations that affect this paper. Another possible approach would be to measure experimentally these effects. This could be a challenging task, but if the effects are as large as suggested by the authors, it should be possible to record surface waves in the soft soil generated by nearby structures. The numerical results presented by Bard & Wirgin indicate that the effort is much worth pursuing. If they are right, these effects would change dramatically our concept of "free-field" motion, and our approaches towards interpretation of strong motion records in an urban environment.

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DISCUSSION ON

"Ground Motion Amplification Using Microseisms" by John M. Ferritto (Paper No 7.09)

By

Mehedi A. Ansary, Graduate Student, Department of Civil Engineering, University of Tokyo.

The above titled paper has discussed the use of microseism measurement as a tool to predict local site microzonation.

This analysis is mainly concerned with soft soil sites and with its amplification characteristics. At first the author described seismic system model and system Identification method in details and later applied the procedure for a specific naval facility site (NFESC site) The spectral ratio of the soft soil site (A13) of NFESC building 582 and a rock site (Laguna Peak) was compared with 1D wave propagation analysis.

Infact this kind of analysis technique using microtremors is very common in Japan. Although in most cases short-period microtremors are used for site characterization, shear-wave velocity determination but long-period microtremors are also used (e.g., Horike, 1985; Kagami et. al, 1986; Tokimatsu, 1992 etc.).

I would like to make several inquiries about the study: whether long term reference measurement have been made in the building itself or at a free-field site close to the structure? What is the distance between Laguna peak and the building? What kind of software has been used for 1D wave propagation analysis?

My suggestion is to use Tokimatsu's (1995) method for accurate determination of shear wave velocity at this site.

The relation between peak rock velocity and spectral ratio as motion changes from strong (e.g., 1989 Loma Prieta Earthquake) to weak (e.g., microseism) is reasonable for soft soil site. At the end I would like to thank the author for sharing his research with us.

Discussion on paper titled: "Ground motion amplification using microseisms", by J.M. Ferrito (Paper 7.09)

By: Francisco J. Chávez-García, Instituto de Ingeniería, UNAM, Mexico.

This paper discusses the use of microseism measurements as a tool to predict local site amplification. The author used measurements at nearby pairs of rock and soil sites and computed spectral ratios. These ratios are identified with the transfer function of soft soils overlying the rock basement, and interpreted based on the equation of a sdof system. Finally, the author compares the relation between peak ground velocity and maximum amplification for microseism with results for the main shock and aftershocks of Loma Prieta earthquake to suggest very large non linear effects in the amplitude of spectral ratios.

There has been some discussion about the applicability of microtremor measurements (see e.g., Aki, 1988). A recent review of the different techniques that have been used to analyze microtremors (including spectral ratios) was presented in Lermo & Chávez-García (1994). It is generally accepted that microtremors are useful in the long period range, but their applicability to higher frequencies is still debated. In this context, it is unfortunate that in this paper there is almost no comment about the frequency dependent characteristics of microtremors. The spectral ratio technique requires, either that the source of excitation of microtremors be the same both for the reference and the soft soil sites, or that microtremor spectra on rock be flat over the frequency range of interest. This issue could have been better discussed, as we have no indication of how far apart are these two sites (reference and soft soil sites). Nor do we know the frequency range at which the author observed the maximum effects.

I would also have liked some information about the recording instrument used. It is apparent that a velocity recording instrument was used (see Figure 3), but we do not know what seismometer was used. In Figure 1, Fourier spectra is plotted as a function of time. The reader is led to think that what is plotted is the maximum amplitude of Fourier spectra modulus as a function of time. In Figure 1, we observe significant variations of amplitude, but we do not know whether maxima occur always at the same frequency. Nor does this figure inform us of the spectral shape of microtremors on rock.

Another comment concerns the statement that, based on the geometry, topographic effect at Laguna Peak site is about a factor 2. We know that topographic effect is frequency dependent. Does this factor of 2, expected from geometry, occur for the same frequency band for which a spectral ratio of 2 was observed? Now, a factor of 2 has been repeatedly (e.g., King & Tucker, 1984; Chávez-García et al., 1990) mentioned as a minimum uncertainty in spectral ratios. Additional comments are required if we are to accept that this effect is significant.

Figure 2 shows contours of average spectral ratios within a frequency band. I would have liked to know which frequency band, whether it corresponds to maximum values observed, and how does the East component compare to North or vertical components. No scales are given, so we do not know what the contour values mean, nor how fast do they change with distance. Given a basic uncertainty of a factor 2, 15% variation in spectral ratio amplitude seems insignificant.

The author estimates a 0.014 of critical damping for site A3, based on the peak amplitude of microtremor spectral ratios. I feel that he should have mentioned explicitly that this is only valid if we neglect radiation damping. Due to this limitation, damping values are probably overestimated, and should be used with care in a wave propagation model.

Finally, as regards the influence of non linear effects for Treasure Island site, this reader finds it difficult to believe that, due to non linear effects, amplification changed from a factor over 60 to a factor below 10 for peak acceleration of 10^{-4} g. I do think that, at least part of the differences come from the large variability of spectral ratios.

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Discussion on paper titled: "Use of Microtremors for the Estimation of Ground Vibration Characteristics", by Mehedi Ansary et. al., Paper No. 7.12

By: J. M. Ferritto Naval Facility Engineering Center, Port Hueneme, California.

The authors present results of microtremor array measurements conducted at five sites in the Tokyo area. The measurement of microtremors involves a deliberate choice of a frequency range of interest. Some researchers have chosen to investigate "long period" motion where the principle source of excitation at coastal locations originates from ocean induced bedrock vibration at periods of 6 and 12 seconds. The authors chose to make short period measurements (0.05 to 1.0 sec) which are caused by traffic noise. Three component velocity measurements were made for 2 minutes every hour for a 24 hour period. The measurements exhibited amplitude variation with time based on the levels of ambient activity. The Fourier spectrum of any recorded signal is influenced by both site response and source noise. In the case of this study, the source was random man-made vibrations from traffic and machine foundation noise. The influence of source was shown by changes in (narrow) peaks when traffic or machinery noise was present while the major (broad) peaks indicative of site response tended to remained more constant. In my opinion, it is difficult to separate these elements of response. Because of this, researchers have used several techniques to better characterize site effects and eliminating source influence. A close-by reference site, usually a rock outcrop, can be used to normalize measurements eliminating source components and developing a transfer function of site amplification. When rock is not present as is probably the case for this case study, a soil site whose properties and response characteristics are known can also be used; measurements then indicate response relative to the reference site. In this way the influence of source can be canceled by computation of a spectral ratio of local site response. The authors instead used a technique originated by Nakamura (1989) in which the horizontal motion is divided by the vertical motion to determine a spectral ratio. Unfortunately the validity of this approach was not discussed. This discusser in his own research has noted that division of the horizontal spectrum by the vertical spectrum did not cancel source effects especially long period excitations emanating from the ocean. It is unclear as to exactly what the resulting spectral ratio does represent. They note that period peaks in the spectral ratio differ from those in Fourier spectrum and the spectral ratios tend to be more stable. This fact has been noted by those who use the technique of a reference site.

This was an interesting informative paper and the authors should be commended for their effort.

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DISCUSSION ON

"The Amplification of Seismic Waves in Tehran" by S.M. Mir Mohamad Hosseini (Paper No 7.17)

By

Mehedi A. Ansary, Graduate Student, Department of Civil Engineering, University of Tokyo.

The above paper presents the investigation result of the influence of different geotechnical parameters of a site on the amplification of earthquake waves. The obtained results are represented by the response spectra in the ground surface to evaluate the effect of each parameter on the earthquake amplification.

The selected site (Fig. 1) belongs to the medical science university of Iran. The response analysis was performed by using 1D wave propagation program SHAKE. For this site it was expected that a near field earthquake of M=7 Richter and horizontal acceleration of about 0.27g is suitable as input motion. For this purpose the record of El-Centro earthquake (1948) was used (Fig. 2). The average values of site parameters are given in Table 1.

For this specific site, it can be concluded that the variation of density, shear wave velocity, shear modulus, damping ratio and thickness of the first layer do not have any influence on the frequency of the response spectra, but except shear wave velocity the variations of other parameters change the amplitude of acceleration (Fig. 3 to Fig. 7).

Finally, I would like to inquire whether the authors have made any dynamic analysis related to linear or non-linear soil response for this particular site and if so what kind of results have been obtained?

The limitation of original SHAKE program in modeling only vertical shear waves mentioned by the author can be eliminated by applying some refinements as proposed by Kausel and Roesset (1984), sothat the program can be used for non-vertically incident shear waves.

Discussion on paper titled "Microzonation Studies for Lake Maracaibo Costal Protection System", by Dr. J. P. Sully, Dr. E. Gajardo, Dr. J. Murria and Dr. J. A. Saab (Paper No. 7.20)

By: Madan B. Karkee, Development Division, GEOTOP Corporation, Tokyo, Japan

In this paper the authors present a summary of the methodology utilized in the seismic microzonation of lake Maracaibo coastal region of Venezuela incorporating a sequence of interdisciplinary investigations and observations carried out over a period of time. The seismic microzonation study are utilized to evaluate the seismic stability and integrity of dykes around the lake, and consists of a comprehensive program for retrofitting, remediation, and extension of the coastal protection system. The seismic microzonation study concerns a region with no strong motion records available from past earthquakes. The writer feels that the case study reported by authors will serve as a valuable background for such attempts elsewhere where similar situations exist.

As the extent of the material covered is quite diverse, some of the points are not clearly evident. The writer would like to note some points for further clarification. (1) The average shear wave velocity is assumed to represent the so called 'similar-type response' profiles. It is not clear if ground response analysis of a number of profiles with the same average shear wave velocity was carried out to verify the validity of this assumption. (2) Peak acceleration, duration and epicentral distance of the incident motion is said to be determined from the seismic hazard analysis and seismotectonic studies. However, the method employed to generate the synthetic motion to represent these values is not mentioned. Also it is not clear what real acceleration records were utilized. (3) The so called 'new bedrock depth' fixed at 50m is said to be an unbiased average based on soil modulus variation and depth to bedrock. Further clarification on how it is fixed would be helpful. (4) In the sensitivity analysis of the peak acceleration at 50m depth due to variation of input bedrock depth from 150 to 500m, the peak input acceleration of motions at different depths are seen to be different (Fig. 3). It seems the input motions are different in each case. A comment on how the individual input motions at different depths were assigned would be very helpful.

Discussion on paper titled "Seismic Response of 2D-valleys: Local Site Effects", by Dr. K. E. Loukakis and Dr. J. Bielak (Paper No. 7.21)

By: Madan B. Karkee, Development Division, GEOTOP Corporation, Tokyo, Japan

The authors present an interesting discussion on the valley effect in the seismic response by utilizing the finite element formulation. The domain of computation is confined by transmitting boundaries represented by dashpots to absorb the scattered waves. Domain decomposition technique is utilized to represent separately the valley region, which can be allowed to behave nonlinearly, and the halfspace region, which is constrained to be elastic or viscoelastic. The formulation is noted to be capable of representing any valley shape and layering system together with the material damping characteristics. However, only simple valley geometry cases, with the incident motion defined by a single half cycle displacement pulse in each case, are considered in this study, and the effect of material damping is neglected. Parametric studies are made made for different pulse durations and angles of incidence. It is concluded that horizontal and vertical displacements are affected by valley geometry, particularly the inclination of valley sides, and that the layering and angle of incidence influence the amplification and duration of ground response.

The finite element formulation presented by authors seems effective in simulating the seismic response of 2D-valley. Even more interesting results may be expected by incorporating the material damping in the analysis. The difference between 2D-response and the corresponding 1D-response considering flat layers is stated to be 'due exclusively to the surface wave generated by the lateral edges'. Considering that the near surface layers are generally very soft, increased damping with higher strain level may result in smaller differences between 2D and 1D-responses when the material damping is considered. Authors' comment in this regard will be very helpful.

Discussion on paper titled: "Seismic response of 2D-valleys: local site effects", by K.E. Loukakis & J. Bielak (Paper 7.21)

By: Francisco J. Chávez-García, Instituto de Ingeniería, UNAM, Mexico.

This paper presents results of an investigation of the effect of 2D geometry on ground response. The authors use finite element method to compute ground response to plane SV wave incidence on different models of a sedimentary valley. Five different models are discussed. In three of them the sediments filling the valley are homogeneous, with different geometry. The two other cases consider horizontal layers within the 2D structure. The authors present some results to validate their method, showing that no spurious reflections occur within their mesh due to the finiteness of the finite element mesh. All the models studied in this paper are quite shallow alluvial valleys (maximum thickness varies between 36 and 45 m, whereas the width of the model is constant, equal to 1140 m) with a very large velocity contrast. S-wave velocity at the surface varies between 30 and 70 m/s, while the half-space has S-wave velocity of 400 m/s.

The results presented in this paper were all computed for perfectly elastic models, as inelastic attenuation was neglected throughout. Such models are useful to get a clear idea of the different effects we may expect from 2D valleys. However, they must be considered with precaution, as it is clear that soft sediments with S-wave velocity as low as 30 m/s must have a Q_S value smaller than 50. This point becomes very important in making specific predictions as shown in Chávez-García and Bard (1994).

Figure 4 deserves an additional comment due to an unfortunate error. This figure compares the 1D with the 2D response of one of the layered valley cases for vertical incidence of S waves. Either the seismograms shown correspond to vertical incidence of SH waves (contrary to what is explicitly stated at the beginning of the section), or there is an error in the 2D seismograms. Even if incidence is vertical, SV waves will generate diffracted motion in the vertical component. Diffracted surface (Rayleigh) waves must have some motion in the vertical component, shown as zero in Figure 4b.

The results by Loukakis & Bielak confirm previous studies of the seismic response of alluvial valleys to SV wave incidence (e.g., Bard & Bouchon, 1980; Kawase & Aki, 1989; Ramos-Martínez, 1992; Sánchez-Sesma et al., 1993). However, and contrary to previous studies, Loukakis & Bielak observe that for oblique incidence the response of the side of the valley nearer to the source is much greater than on the opposite side. Ramos-Martínez (1992) arrives to the opposite conclusion; it is the side further from the source that experiments the largest amplifications of ground motion. It is to be hoped that this difference comes from the different models computed by each author, and not from a numerical problem in either method. If this difference comes from the particular model used for the computations, there is little hope to incorporate these differences in ground motion estimation for future earthquakes. In this sense, the strong dependence of ground motion on incidence angle shown by Loukakis & Bielak suggest that we are still far from being able to incorporate 2D effects into simple microzonation schemes.

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Discussion on paper titled "Estimation of Local Site Conditions in Kushiro City Based on Array Observation of Microtremors", by Dr. K. Tokimatsu and Mr. H. Arai (Paper No. 7.22)

By: Madan B. Karkee, Development Division, GEOTOP Corporation, Tokyo, Japan

In this paper, the authors discuss the suitability of utilizing the inverse analysis of Rayleigh wave dispersion curves inferred from the F-K spectrum analysis of microtremors measured by an array of sensors to estimate the shear wave velocity profile of ground to a depth of 300m. The discussion is based on the comparison of observed and computed spectrum ratio between two earthquake strong motion recording sites in Kushiro, Japan located about 4 km apart. The authors have employed improved equipment for field measurement together with the in-house analysis system that they have developed to show that the array observation of microtremors has a promising potential for estimating subsurface ground conditions in a very cost effective manner.

The writer feels that the paper is a step toward improving our ability to reliably infer the shear wave velocity profile of ground without the need of a borehole. However, some comments on the comparison between observed and computed spectrum ratio (Fig. 9) are in order.

Compared to the observed spectrum ratio, it seems the computed spectrum ratio is clearly small in the short period (less than about 0.15 sec) and is noticeably large in long period (greater than about 0.7 seconds) range with closer agreement in between. If the ground response is considered to be linear, it may be reasonable to assume that the ground response at short period reflects the ground condition details close to the surface. From this standpoint, it can be reasoned that the smaller computed spectrum ratio in short period range may have resulted from difficulty in the representation of near surface details in the shear wave velocity profile inferred. This may be particularly so at site H which is located close to the bay area, presumably underlain by soft layers to a depth of about 60m. Conversely, owing to dissimilar local soil conditions at the two sites, there may have been different extent of nonlinearity in the observed ground response, contributing to the difference between observed and computed spectrum ratio. The strong motion records may also contain soil-structure interaction effects, particularly at site J where the M7.8 event of 1993 is shown by Dan (AIJ general symposium on the 1993 Kushiro-oki earthquake, December 2, 1994) to have displayed clear evidence of interaction effects. It would be helpful if the authors could comment on these aspects.

DISCUSSION ON

"Estimation of Local Site Conditions in Kushiro City Based on Array Observation of Microtremors" by Kohji Tokimatsu and Hiroshi Arai (Paper No 7.22)

By

Mehedi A. Ansary, Graduate Student, Department of Civil Engineering, University of Tokyo.

In this paper short-period microtremors measurements are conducted at two strong motion stations to find its use for estimation of the effects of sub-surface soil condition on the ground motion characteristics. The important finding of this paper is the estimation of shear-wave velocity by the inversion analysis of the Raleigh wave dispersion curve. So instead of making costly in-situ measurements the quick, inexpensive and non-destructive microtremor method can be used.

In this method the authors established that for medium to some deeper depth (depending on wave length) microtremor measurement is effective and for shallower depth Stokoe and Nazarian (1984) SASW method have to be used in conjunction with this.

The authors also showed using the inversed soil profile that computed and observed spectrum ratio show fairly good agreement, emphasizing that the above estimation of soil profile is economical yet reliable.

I would like to make several inquiries: whether amplitude ratio shown in Figure 3 and 6 is for fundamental mode or superposed modes of Raleigh wave? If possible some explanation of the term "medium response" as mentioned in Tokimatsu (1992) is needed. What is the relation between the spectrum ratio of strong ground motion and microtremor between these two sites ?

Authors Response on paper titled:
"Ground motion amplification using microseisms" (Paper 7.09)

By: J. M. Ferritto

The author thanks Mr. Ansary and Professor Chavez-Garcia for their most interesting comments. The microseism source used for this work consisted of ocean induced vibration of 7 and 14 second period. The source motion was the same at both soil and rock reference site. This was demonstrated in the spectral ratio which demonstrates complete cancellation of the source in put by having a minimum ratio at period range over 5 seconds. Figure 1 shows variation of the 2-4 second spectral average segment with time. Figure 2 of the paper shows contours of a portion of the spectra between 2 and 4 seconds as a typical illustration of what could be done to illustrate spatial variation. Other period segments could have been displayed but we were limited by publication constraints.

The data shown in Figure 3 for main shock and aftershocks are from Darragh and Shakal (1991). The microseism data were recorded as part of this study. Additional information on the Gilroy site and other soft sites shows similar behavior. The response shown is typical of soft sites only. Only soft sites exhibit the increase in amplification with decreasing levels of excitation. Ten pairs of sites from the 1994 Northridge event on stiffer geologies do not exhibit the same relationship but rather appear to have the same relatively constant level of amplification for both main shock and aftershocks. The amplification associated with soft sites is explained in terms related to the shear strain level such that soft sites exhibit a smaller drop of shear modulus and lower levels of damping with increases in shear strain. Thus the response exhibits a more elastic material behavior. The topic of nonlinear behavior is really not an issue here; nonlinear site response has been recognized as early as 1972 when engineers started to use strain dependent material properties.